On the Internal Fluidity of the Earth

THE question of the solidification of the crust of the earth from the fluid interior nucleus, as referred to by Mr. Mathieu Williams, in NATURE, vol. xv. p. 5, is one which has been long since fully discussed in my papers in the *Philosophical Transactions*, the *Atlantis*, vol. i., and in a paper of which an abstract appears in the Report of the British Association for 1856. As far as I am aware, no person has controverted my conclusions as to the process of solidification of the earth. The results are, in the main, somewhat similar to those so admirably illustrated and enforced by Mr. Robert Mallet, and also such as Mr. Williams upholds in this journal.

In articles 6 and 7 of my "Researches in Terrestrial Physics," Part 1, this subject is discussed as a problem of fluid equilibrium, and the conclusion is there deduced that the fluid interior mass of the earth must consist of spheroidal strata of equal density, the density of each stratum increasing from the surface to the centre of the nucleus. The mode in which this arrangement of the fluid matter would favour the formation of a solid crust is pointed out. In Part 2 Section III., the probable law of density of these fluid strata is discussed. In Section IV. the shape of these strata is investigated, and also that of the inner surface of the shell or crust. It is shown independently of the law of density that the least ellipticity of this inner surface of the crust cannot be less than the ellipticity of its outer surface. A similar result was soon afterwards enunciated by Plana in a paper in the *Astronomische Nachrichten*. In the same section the theory of a solid nucleus in the earth originally proposed by Poisson, is examined and shown to be incompatible with physical laws.

Owing to the pointed manner in which Sir William Thomson invited discussion in a previous number of NATURE, I ventured to controvert his views as to the rigidity of the earth in a paper inserted at p. 288, vol. v. of this journal. Never at any time have I had even a doubt as to the untenable character of Sir William Thomson's views regarding the solidity of the earth.

In again reiterating this opinion in NATURE, vol. ix. p. 103, a reference to my paper was given, in which vol. vii. p. 288, is misprinted for vol. v. p. 288.

In his address at Glasgow Sir William Thomson, while maintaining his opinion as to the earth's solidity, appears to have seen the weakness of some of his former arguments by calling on his hearers (NATURE, vol. xiv. p. 428) to erase whole paragraphs of his paper on the Rigidity of the Earth, in the *Philosophical Transactions*. At the same passage of his address he refers to a hint from Prof. Newcomb, that viscosity might suffice to render precession and nutation, the same as if the earth were rigid. "This," he says, "I would not for a moment admit, any more than when it was *first* put forward by Delaunay." The *Comptes Rendus* of the Academy of Sciences of Paris for March 6, 1871, contains a paper in which my priority on this point is clearly established. In NATURE, vol. iii. p. 420, the following statement occurs:—"Paris Academy of Sciences, March 13.—M. Delaunay read a declaration stating that he acknowledged that Mr. Hennessy had used the same arguments as himself against Mr. Hopkins' theory relative to the fluidity of the interior parts of the earth."

I am willing to believe that Sir William Thomson had neither seen the *Comptes Rendus* nor the paragraph in NATURE just quoted, but it is to be regretted that a presidential address should contain an erroneous statement on a point of recent scientific history, especially when the error could be avoided by a glance at the most widely known scientific publications.

All through the portion of his address which refers to the earth's structure Sir William Thomson assumes that the views of Mr. Hopkins are established and admitted. A reference to some of the past volumes of this journal alone shows the inadmissibility of such an assumption. At pp. 45 and 182 of vol. iv. and elsewhere Mr. Hopkins' views are distinctly controverted on mechanical, physical, and geological grounds.

It appears that in the discussion on my paper in the Academy of Sciences of Paris, in which some of the most eminent mathematicians and geologists of France took part, not one of them adopted Mr. Hopkins' "Discovery of the earth's solidity." As far as I am aware, this "discovery" is not adopted anywhere on the continent of Europe. I have studied with as much care and attention as I could give to them, the mathematical and physical researches of Mr. Hopkins and Sir William Thomson relative to this subject, and for reasons already partly unfolded in this journal at vol. v. p. 288 and vol. iv. p. 182, I continue to firmly adhere to the almost diametrically opposite conclusions long since enunciated in the publications referred to at the outset of this communication.

HENRY HENNESSY

Royal College of Science, Dublin

The Age of the Rocks of Charnwood Forest

I SEE that in Mr. Woodward's "Geology of England and Wales," recently published, the rocks of Charnwood Forest, in Leicestershire, are (with some hesitation) referred to the "Laurentian" series, Prof. Ansted and Dr. Holl being quoted as authorities. The reviewer in the recent number of the *Saturday Review* adopts the same opinion; at the same time it is proper to add that Mr. Woodward states in another place (p. 31) that the Charnwood Forest rocks "may be of Cambrian age," so that the reader is left to take his choice.

For my part I confess to being at a loss to understand on what grounds these old rocks can be referred to any other than the Cambrian period. The evidence in any case is small, but what there is points to this conclusion.

In the first place it ought to be remembered that the age of these rocks was first indicated by Prof. Sedgwick, whose opinion on such a question should not be disregarded unless on very substantial grounds. Sedgwick's opinion of their age was founded almost entirely on lithological grounds, and no one was better qualified to recognise the representatives of the Welsh Cambrians, though rising up isolated amongst much newer formations in the heart of England. Prof. Jukes, in his description of the geology of Charnwood Forest, appended to Porter's "History," adopted the same view, in which the officers of the Geological Survey, including the present Director-General, who made a personal examination of the forest rocks, concurred (see "Geology of the Leicestershire Coal Field," Mem. Geol. Survey, 1860). I am not aware that they have changed their views owing to what has been since written on the subject.

As regards the determination of the age of these rocks, if it is impossible to prove them to be of Cambrian age, there are very good grounds for concluding they are not of "Laurentian" age, assuming that term to refer to the fundamental gneiss of the north-west Highlands and Isles of Scotland. These rocks consist, according to the description of Murchison, of coarsely crystalline gneiss, full of granite veins. They are everywhere intensely metamorphosed. Now, this is far from being the case with the Charnwood Forest rocks. Generally they are no more metamorphosed than are the Cambrian beds of the Longmynd, or of Llanberis. True "gneiss" is very exceptional, and metamorphic action is quite local, and is chiefly confined to one district. Any argument, therefore, drawn from lithological resemblance to the rocks of the typical district entirely fails; and I cannot admit that the occurrence of rocks (syenite, &c.) resembling those of the Malvern Hills, is of any force in this question, as it is very far from having been proved that the Malvern Rocks are of Lanrentian age.

As regards evidence founded on organic remains, it is of the most meagre kind, but whatever the obscure markings on the slates of Charnwood may really be, they are certainly not those of *eozoon*. From whatever side, therefore, the question is viewed, there appears to be no good ground for departing from the view regarding the age of these rocks originally adopted by Sedgwick.

EDWARD HULL

Geological Survey Office, Dublin, November 3

Mind and Matter

MR. SPALDING in his critique on Maudsley's "Physiology of Mind" (NATURE, vol. xiv. p. 541), while admitting that "the dependence of consciousness on nervous organisation, seemed," by the science of nerve physiology, "to be fairly established," stated that the difficulty of conceiving *how* consciousness stood related to the material organism, was a difficulty which had not yet been overcome.

Might not this problem be solved somewhat thus:—It is as easy to predicate subjectivity (or susceptibility to consciousness) of one entity called matter, as of another entity called soul or spirit. It is no more difficult to conceive of matter being subjective than of spirit being subjective.

Again, energy accompanies matter in all its forms, and yet

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how it is related to the entity called matter, is no less mysterious than how subjectivity may be a property of matter. Energy moreover may be divided, Why may not subjectivity? Energy exists in isolated as well as in grouped and combined forms, Why may it not be so with subjectivity? Energy exists in a potential form when opposing forces neutralise each other—that condition of matter we call rest. May not subjectivity exist in a potential form when opposite kinds of subjective states tend to establish themselves in the same material mass—so constituting that condition of matter which we call unconsciousness? Energy is only kinetic or active, that is, only shows itself in the form best realised by us when one force has of all the others the ascendency, or is the expression of their united tendencies. May it not be so of subjectivity that it only develops into its active and best recognised form when one kind of subjective state has the ascendency, or is the expression of the united subjective states? Thus, as energy potential is rest, so subjectivity potential is unconsciousness. As kinetic energy is motion, so active subjectivity is consciousness.

In this way while all matter is subjective or susceptible of consciousness, this subjectivity seems to exist in the potential form only in all but organisms possessed of a nervous system. In the active nerve fibre the subjectivity of matter appears alone to be active or conscious, while the complex organisation of the nervous systems of the higher animals alone permits of matter rising to the powers of mind by harmoniously combining many subjective states so as to build them up into perception, understanding, memory, imagination, reason, invention, and judgment.

WM. S. DUNCAN

Stafford, November 9

Meteor

I OBSERVED from a high point overlooking the Weald, on the night of November 6, about the time Mr. Nostro mentions, a large meteor fall from a point a little below the zenith in the northern sky. It burst twice, emitting bluish sparks in doing so, once shortly before it disappeared, and the second time on its disappearing. Could it have been the same meteor seen from different positions?

I could not be positive to a point or two as to its exact magnetic bearing, but I do not think I am far wrong in saying it fell almost due north from where I observed it.

CECIL H. SP. PERCEVAL

Pulborough, November 18

THE PRESENT STATE OF MATHEMATICAL SCIENCE

AT the meeting of the Mathematical Society on November 9, Prof. H. J. S. Smith gave an address on this subject, in which he excluded all reference to applied mathematics. "I shall regard it," he said, "as a fortunate circumstance if my successor when he, in his turn, is looking round for a subject for his own presidential address, should be attracted by a domain on which I must myself decline to enter, but of which he, better perhaps than anyone among us, is fitted to give us a clear and comprehensive view." He professed to offer only fragmentary remarks, "hoping that even such fragmentary remarks may not be without their use if they serve to remind us of the vastness of our science, and yet of its unity; of its unceasing development, rapid at the present time, promising to be still more rapid in the immediate future, and yet deriving strength and vitality from roots which strike far back into the past, so that the organic continuity of its gigantic growth has been preserved throughout. In every science there is a time and place for general contemplations, as well as time for minute investigations. And it is a rule of sound philosophy that neither of these shall be neglected in its proper season ('itaque alternandæ sunt istæ contemplationes,' says Lord Bacon, 'et vicissim sumendæ ut intellectus reddatur simul penetrans et capax')."

Touching upon a charge brought against the *Proceedings* of the Society that its memoirs "have shown and still continue to show a certain partiality in favour of one or two great branches of mathematical science to the comparative neglect and possible disparagement of others," it might be rejoined "with great plausibility that ours is not a blamable partiality but a well-grounded preference. So great (we might contend) have been the triumphs achieved in recent times by that combination of the newer algebra with the direct contemplation of space which constitutes the modern geometry—so large has been the portion of these triumphs, which is due to the genius of a few great English mathematicians—so vast and so inviting has been the field thus thrown open to research, that we do well to press along towards a country which has, we might say, been 'prospected' for us, and in which we know beforehand we cannot fail to find something that will repay our trouble, rather than adventure ourselves into regions where, soon after the first step, we should have no beaten tracks to guide us to the lucky spots, and in which (at the best) the daily earnings of the treasure-seeker are but small, and do not always make a great show, even after long years of work. Such regions, however, there are in the realm of pure mathematics, and it cannot be for the interest of science that they should be altogether neglected by the rising generation of English mathematicians. I propose, therefore, in the first instance, to direct your attention to some few of these comparatively neglected spots."

The foremost place is assigned, by Prof. Smith, to the Theory of Numbers. "Of all branches of mathematical knowledge this is the most remote from all practical application, and yet, perhaps more than any other, it has kindled an extraordinary enthusiasm in the minds of the greatest mathematicians. We have the examples of Fermat, of Euler, of Lagrange, Legendre, of Gauss, Cauchy, Jacobi, Lejeune Dirichlet, Eisenstein, without mentioning the names of others who have passed away, and of some who are still living. But, somehow, the practical genius of the English mathematician has in general given a different direction to his pursuits; and it would sometimes seem as if we measured the importance of the subject by what we find of it in our best treatises of algebra, or as if we accepted the denunciations of Auguste Comte, and regarded the votaries of the higher arithmetic as reprobate of positive science, as moving in a vicious circle of metaphysical ideas, and as guilty of a great crime against humanity in the pursuit of knowledge beyond the limits of the useful. . . . I would rather ask you to listen to what is recorded of the great master of this branch of science."

Gauss (we are told by his biographer) held mathematics to be the queen of the sciences, and arithmetic to be the queen of mathematics—"She sometimes condescends to render services to astronomy and other natural sciences (so spoke the great astronomer and physicist); but under all circumstances the first place is her due." A citation was also made from Jacobi's Life of Göpel: "Many of those who have natural vocation for pure mathematical contemplation find themselves in the first instance attracted by the higher parts of the theory of numbers."

Three great departments of arithmetic were instanced: The arithmetical theory of homogeneous forms (or quantics)—"It is a memorable fact that some of the greatest conceptions of modern algebra had their origin in connection with arithmetic, and not with geometry or even with the theory of equations." In the "Disquisitiones Arithmeticæ" are given for the first time the characteristic properties of an invariant and a contravariant (for ternary quadratic forms). "But the progress of modern algebra and of modern geometry has far outstripped the progress of arithmetic; and one great problem which arithmeticians have now before them is to endeavour to turn to account for their own science the great results which have been obtained in the sister sciences. How

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